Wind Energ. Sci. Discuss., referee comment RC1
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# Comment on wes-2021-110

Anonymous Referee #1

Referee comment on "Surrogate models for the blade element momentum aerodynamic model using non-intrusive polynomial chaos expansions" by Rad Haghi and Curran Crawford, Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-110-RC1, 2021

## General comments

The paper addresses an important and challenging topic in the application of stochastic mathematical tools to engineering problems. The application is meant to alleviate computational cost issues and enable future analysis with more complex models. The motivation is clear and the research is relevant for the corresponding field of application.

First, the authors introduce the topic and motivation, highlighting the fact that complex wind turbine aerodynamic models need to account for a large enough input space, making the problem of building a PCE affected by the curse of dimensionality. Furthermore, they argue that intrusive PCE models are off the table with the foresight of using commercial codes and complex computational models in the future. All of the above is motivation enough to pursue the presented research, namely, building a non-intrusive PCE model on a simplified (or reduced) Veers model for unsteady wind generation. The details of the models for wind generation and aerodynamic loads are introduced next together with how the statistical convergence will be assessed which is important for the simplification of the methodology. The PCE, random sampling and approach followed are introduced next. In the results section, the authors discuss the Hellinger distances computed and assess the PCE accuracy with varying degree of polynomials as well as their efficiency compared to the reference case. The paper ends with conclusions and discussion of future developments.

**Specific comments**

Several questions and concerns are raised.

The relevance of the work presented here rests on three key aspects:

- The non-intrusiveness of the approach, given that more complex models are to be used in the future with this framework.
- The alleviation of the curse of dimensionality of PCEs which is relevant for efficient surrogate building, especially with the foresight of using more complex models as well.
- Showing that statistical convergence is achieved which is an argument to be used to simplify the surrogate building process.

However, there are issues with two of those three key aspects that would need to be addressed by the authors before releasing this work. First, the curse of dimensionality is not adequately alleviated for the future purposes of this work. Selecting a simpler wind velocity model with a reduced number of inputs does not tackle the problem, just avoids it. When a more complex model is needed, the same technique presented here is not enough to tackle the challenge of dimensionality in PCEs and the work should be revisited entirely. The literature is very rich with mathematical ways of dealing with this issue (low-rank approximations, sparse PCEs etc) and these have not been explored which would be more important for the impact of this work in the field of wind engineering. Furthermore, there are no account of errors for each PCE approximation, for instance for Figure 4, computations of the L2 error norm, not just a qualitative indication.

The statistical convergence is assessed using only one metric without giving enough motivation as to why that metric and what errors are committed when using it (how good of an assessment does the Hellinger distance give you? Could it give you a good assessment even in cases that are clearly not good?). It would be interesting to round up that part of the work with another widely used metric, the Kullback-Leibler divergence, used in surrogate modeling when approximating models for Bayesian analysis, for example. Furthermore, when using more complex models, the argument of statistical convergence may not hold due to new underlying physics, so how should we take this?

An interesting way of enriching this work and properly tackling those three key aspects would be by adopting both the reduced Veers and the full Veers model and make a comparative study with different techniques to build the PCEs and show how you could efficiently used these techniques with a baseline model (reduced Veers) and a more complex one without having to trade some of the physical aspects.

Another concern to be raised is the structure of the paper. It is not well-organized and some sections are a paragraph long. Maybe a better way would be to devote section 2 entirely to the models the authors aim at approximating, with more in-depth discussion. Section 3 could be devoted to the methodology and all the different aspects. Section 4 could then discuss the results.

**Technical corrections**

Find a list of typos found (non-exhaustive):

- Page 4, line 84: dimensionality
- Page 4, line 97: properties **are** extracted
- Page 6, line 123: orthogonal **polynomials**
- Page 7, line 173: **collocation**
- Page 9, line 214: **Hellinger**
- Page 12, line 273: we use **the** Hellinger distance

There are also English inconsistencies throughout the text, with sentences missing verbs etc. The authors should re-read the manuscript carefully.